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The above reference to the Starling in Dr. Shufeldt's paper, taken with other passages in the same article, clearly reveals the animus of his critique.

J. A. ALLEN.

'WHAT IS TRUTH?'

In all our speculations concerning nature what we have to consider is the general rule. For that is natural which holds good.

Aristotle, Parts of Animals III., II., 16.

Knowledge is a double of that which is.

Mr. Bacon in Praise of Knowledge.

Nature means neither more nor less than that which is.

Huxley, VII., p. 154.

If the author of the letter on 'The Material and the Efficient Causes of Evolution' (SCIENCE, p. 668), will refer to an article which the Editor asked me to give him, and printed in SCIENCE in February, 1895 (Vol. I., No 5, p. 125), I think he must admit that I, at least, have not committed the blunder which he lays to the charge of certain unspecified 'Neo-Darwinians' and 'Neo-Lamarekians,' and that there is no just *cause or reason* why my name should be dragged into print in this connection.

However, I heartily agree with him that rigorous exactness is necessary in the use of philosophical language; and I also agree with him that, when no qualification is used, or implied, the English word *cause* should mean 'that which produces a thing and makes it what it is;' although it is one thing to define a word and quite another thing to show the existence of any corresponding reality.

As I am advised by this writer to consider Aristotle and be wise, I refer the reader to the passage I have put at the top of this letter, for it shows that this great naturalist is in accord with Bacon and Huxley in the opinion that our business in this world is to learn all we can of the *order* of nature, leaving to more lofty minds the attempt to find out what it is that 'produces a thing and makes it what it is,' and every other 'necessary condition of truth' except evidence.

This correspondent says the word *conceive* is not used with precision in my assertion that, evidence seeming adequate, I believe things which I cannot conceive. As Huxley has never

been accused of inexactness in the use of words I call attention to the following passages which show that this cautious thinker also believed what he could not conceive.

"I cannot conceive how the phenomena of consciousness are to be brought within the bounds of physical science," IX., III., 122.

"I believe that we shall, sooner or later, arrive at a mechanical equivalent of consciousness, just as we have arrived at a mechanical equivalent of heat," I., VI., 191.

W. K. BROOKS.

MAY 4th, 1896.

THREE SUBCUTANEOUS GLANDULAR AREAS OF
BLARINA BREVICAUDA.

TO THE EDITOR OF SCIENCE: Though the subcutaneous glands in *Soricidae* have received much attention, these structures are not so well known in all details that further observations on the subject can be considered superfluous.

In examining perfectly fresh individuals of the common short-tailed shrew, *Blarina brevicauda*, taken in midwinter, when glandular development or activity is presumably less evident than it becomes during the rut, I find three large glandular areas—a lateral pair and one infero-median.

On each side of the body, midway between the fore and hind limbs, may easily be recognized a glandular area, half an inch long and one-half as wide, in part overlying the posterior border of the thorax, and thence extending over the abdomen. This is observable without dissection; for, on blowing aside the long hairs which cover it, the space appears to be naked, though it is in fact clothed with short adpressed colorless pelage, like that on the dorsum of the manus. Small flakes of the inspissated secretion may be noticed; but the glandular orifices are too minute to be made out, even with a hand lens, though these may become more readily discernible at another season. Nor is any musky odor perceptible in the present specimens.

The third glandular area of this shrew is larger than the lateral ones, and this is the fact to which I may direct particular attention. This additional patch is situated on the median line of the belly, opposite the lateral tracts, and

extends three-fourths of an inch caudad from the end of the sternum. In outward aspect this tract is identical with the others. On raising the skin the glandular structure is very evident; it is the same in appearance, under the lens, as that of the lateral tracts, but thicker as well as more extensive.

All three tracts are strictly subcutaneous, and come away from the subjacent parts when the skin is raised. They are supplied by large cutaneous vessels, the ramifications of which are conspicuous beneath the integument. This vascularity reddens the minutely granular texture of the glands, which a low magnifying power discloses. The three areas appear alike in both sexes.

ELLIOTT COUES.

WASHINGTON, D. C., May 7, 1896.

INSTINCT.

EDITOR SCIENCE: It seems to me that it would be well to keep the issue with which this discussion started in view, and then the direction in which the truth lies will be clearer. Nothing could be more explicit than the statement by 'The Writer of the Note' in SCIENCE of February 14th, which was this: "A chick will peck instinctively, but must be taught to drink. Chicks have learned to drink for countless generations, but the acquired action has not become instinctive."

In other words, the view that eating is instinctive and drinking is not, was that taught by Prof. Morgan and endorsed by 'The Writer of the Note' in a subsequent communication. Feeling that an important truth was being imperilled, I advanced facts to show that such a view was untenable. This was followed by the recital of additional facts by others, so that it was plain to myself—more so than ever—that such a theory as that first advanced was not sound. I was aware that all three of the writers supporting this view were in accord, constituting a sort of trinity in unity; there was, nevertheless, a great lack of harmony which seemed to be owing to the somewhat important defect that their views were not endorsed by Nature.

Now, to my surprise, Prof. Baldwin claims that I have missed the real point which he takes to be that an instinct may be only 'half congenital,' and cites this drinking of chicks;

but according to the above quotation drinking is not instinctive at all, so that it looks as if the shoe was on the other foot.

In 1894, in a paper read before the Roy. Soc. Can. on 'The Psychic Development of Young Animals,' published in the Proceedings of the Society for 1895 and a copy of which was forwarded to Prof. Baldwin, I emphasized the conception that instinctive acts are *never perfect* at first, or, as Prof. Baldwin would prefer to say, are only partially congenital, though whether such an expression as 'half congenital' is a valuable addition to the English language, I doubt. Now it would be strange that I should alter my own views without noting the change, and miss the point in a matter which I was, I think, the first to emphasize; in fact, I have in this very correspondence in SCIENCE urged this view—the imperfection of instincts. If Prof. Baldwin and those he professes to interpret will grant that eating and drinking in chicks are instinctive; that both alike are imperfect at birth; that congenitally the chick is in the same condition to all intents and purposes as regards eating and drinking, he will, I believe, be in accord with the facts, and we shall all agree that the much overlooked imperfection of instincts is well illustrated by the subjects under discussion, but I should like to add, universal in its application, though in varying degree, the imperfection being in some cases not very obvious to our inadequate observation.

But in discussing evolution I feel that we are on a different plane. Here the appeal to facts is of a much less decisive character.

I have been trying since reading Prof. Baldwin's letter in SCIENCE of May 1st, in reply to my own, to ascertain his real views in regard to evolution, and have some hesitation in deciding whether I really grasp his meaning or not. However a few concrete cases may make matters plainer. A and B are, let us suppose, two individuals that survive because they can and do adapt to the environment; X and Y die because they cannot; or in Prof. Baldwin's terminology, A and B adapt to their 'Social Heredity' constituting 'organic selection' which is ontogenetic or affects the individual. But the survival of individuals specially adapted affects the race or phylum. But surely an indi-